The Republic vs. The Collective: Two Histories of Collaboration and Competition in Modern Science

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Die Republik gegen das Kollektiv: Zwei Geschichten von Kollaboration und Konkurrenz in der modernen Wissenschaft

Kollaboration und Konkurrenz gibt es in der Wissenschaft zwischen Individuen oder verschiedenen Gruppen, größeren Organisationen, Schauplätzen und Nationalstaaten. Die Spannung zwischen individuellem Ansehen und Gruppenmeriten oder individuellem Ehrgeiz und Gruppenleistung ist der wissenschaftlichen Arbeit inhärent und trägt zu ihrem Erfolg bei. Die Autorin vergleicht zwei soziale Modelle der Wissenschaft, die entwickelt wurden, als Wissenschaftler im 20. Jahrhundert zunehmend begannen kollaborativ zu forschen: Michael Polanyis individualistische Freie-Markt-Republik der Wissenschaft und Ludwik Flecks Denkkollektiv. Diese beiden Modelle sollten Praktiken beschreiben und Ideale für die Wissenschaft im allgemeinen auf der Grundlage der Erfahrungen spezialisierter Forschungsgruppen vorschreiben. Die Arbeitsgruppen von Linus Pauling und Dorothy Crowfoot Hodgkin dienen hier der Erläuterung der beiden Modelle. Die Autorin untersucht verschiedene Auswirkungen von Paulings und Hodgkins Praktiken auf das persönliche Ansehen des Direktors und der Mitarbeiter, und schließt mit der Frage, ob eine kollektive Wissenschaft möglich ist.

Schlüsselwörter: Kollaboration, Michael Polanyi, Ludwik Fleck, Linus Pauling, Dorothy Crowfoot Hodgkin

Keywords: Collaboration, Michael Polanyi, Ludwik Fleck, Linus Pauling, Dorothy Crowfoot Hodgkin

Competition and collaboration in science occurs between individuals, within closely-knit teams, between different groups, and across larger organizations, locales, and nation-states. There are many means of describing and analyzing these mechanisms in the sciences, including the increase of teamwork and co-authorship since the early development of scientific societies and scientific periodicals. The balancing act between individual credit and shared ownership of discovery has been at the heart of scientific activity since the seventeenth century when solitary authorship was usual, even among members of the scientific societies.¹ Indeed, only approximately two per cent of all scientific papers during the period from 1665 to 1800 were co-authored. In contrast, collaboration as defined by co-authorship increased first among eminent scientists in France during the period from 1800 to 1830 and then elsewhere,

particularly in Germany, by the end of the nineteenth century, with the chemical sciences leading the way (Beaver et al. 1978: 73, 1979: 237, 233). By the early 1960s, 80 per cent of all papers in chemical science were multi-authored, in contrast to 20 per cent around 1900 (Price and de Solla 1963: 86–91; Clarke 1964). This increase in collaboration and co-authorship was one of the great transformations in twentieth-century science.

With the rise of group research and the increase in co-authored announcements of discoveries, it is not surprising that by the 1930s and 1940s some scientists drew upon their experiences in order to reflect on mechanisms of work and credit within their own research groups and in the broader scientific community. Two important examples are the physical chemist Michael Polanyi's idea of a competitive free-market "republic of science" and the immunologist Ludwik Fleck's idea of the communal "thought collective" of science. These two images have provided scientists, historians, sociologists, and science policy advisors with valuable insights and provocative questions about the way that science has worked in the past or might best work in the future (Polanyi 1941, 1943; Fleck 1935a). In the book *Michael Polanyi and His Generation: Origins of the Social Construction of Science* (Nye 2011), I analyzed some of this discussion, while focusing on Polanyi's scientific research career and his economic, philosophical, and social writings.

Whereas the free-market republic and the thought collective are two apparently contrasting images of scientific practice, it seems likely, or even self-evident, that each image contains only half the story: historical studies suggest that good science has resulted from individualist competition in equilibrium or tension with collective collaboration, even as organized group research and collective reports of results became more usual. It seems plausible that the two different images might be embodied in particular cases of research groups, rather than applied only to the scientific community broadly construed. If we look at microcosms of laboratory practice, some research groups or laboratories may exemplify characteristics of the individualist versus collective poles in significant ways. Two such striking examples are the research laboratories led at mid-twentieth century by Nobel Prize winners Linus Pauling (Nobel Prize in Chemistry 1964) and Dorothy Hodgkin (Nobel Prize in Chemistry 1964), each a pioneer in the study of the structure of large molecules using crystallographic methods.

In what follows, we first look at some of the major features of the "republic" and the "collective" as described by Polanyi and Fleck. We then turn to the research groups of Pauling and of Hodgkin, where the tension between competition and collaboration, individual credit and group credit, or individual ambition and emphasis on group achievement took rather different forms. In conclusion, we examine some of the implications of the individualist and collective practices of Pauling and Hodgkin for their personal legacies and

their research groups, concluding with the question of whether a research group can act as a truly collective collaboration.

Polanyi and Fleck: Free-Market and Collective Images for Science

Polanyi (1891–1976) and Fleck (1896–1961) were men of the same generation. Both were natives of the central Europe of the Russian and Austro-Hungarian empires.² Polanyi grew up in Budapest and completed a medical degree in 1913, followed by a doctoral degree in physical chemistry in 1917 at the University of Budapest. As a result of political anti-Semitism in a new national Hungarian government, Polanyi left for Karlsruhe in 1919 and then took up a post in Berlin in 1920 at the Kaiser Wilhelm Institute for Fiber Chemistry. He became director in 1923 of the chemical kinetics research group in Fritz Haber's renowned Institute for Physical Chemistry and Electrochemistry. Forced to leave Germany in 1933 because of anti-Semitic measures under the new Nazi government, Polanyi spent the rest of his chemical career as Professor of Physical Chemistry and laboratory director at the University of Manchester, before turning in the late 1930s to political and philosophical writings (Wigner et al. 1977; Scott et al. 2005; Nye 2011).

Fleck lived and worked in Lviv (also known as Lemberg or Lwów), which became part of Poland after the Great War. Like Polanyi, Fleck's studies were interrupted by military service during that war. After receiving his medical degree at the University of Lviv in 1920, he became an assistant to the typhus specialist Rudolf Weigl. Similarly to Polanyi, Fleck's post at Lviv was terminated in 1923 because of anti-Semitism. He then became head successively of two different bacteriological laboratories at the Lviv General Hospital, where he collaborated and co-authored publications with colleagues, while he also did routine analyses and research in his own private bacteriological laboratory (Cohen et al. 1986; Löwy 1990; Werner et al. 2011; Sady and Wojciech 2012; Kleeberg et al. 2014; Fagan 2009: 277–278). In 1935, however, he was dismissed and in 1937 deprived of his membership in the Association of Polish Physicians because he was Jewish. He survived the camps at Auschwitz and Buchenwald, where he was forced to work on a typhus vaccine, before returning to Poland, where he completed a Habilitation in 1946. He then directed Departments of Medical Microbiology at universities in Lublin and in Warsaw before leaving in 1957 for academic positions in Israel (Schnelle and Thomas 1986; Grzybowski 2012; Sak et al. 2012; Allen and Arthur 2014).

During the late 1920s and the 1930s Polanyi and Fleck each began to reflect systematically on the nature of scientific life, the character of scientific discovery, and the roots of scientific progress. Polanyi's notion of a "republic of science" was well developed before he published it in 1962 in the journal

Minerva. Its first expression appeared in 1928 in a tribute to Fritz Haber's leadership of his Berlin institute (Polanyi 1928, 1962). In essays in the early 1940s, drawing upon classical economic language, Polanyi sought to discredit science planning and centralized control of science by describing the natural "dynamic" organization of science, using the language of the free-market and of "spontaneous mutual adjustments" of competing ideas within scientific practice. He also described the social and psychological roots of scientific practice, emphasizing long periods of apprenticeship for novice scientists and the necessary acceptance and mastery of dominant interpretive frameworks or traditions in the scientific discipline. Expertise could be acquired, he wrote, "only through the medium of personal collaboration" (Polanyi 1941: 431–432, 435, 1943, 1951: 56–57, 1958 2014, 1966 2009; Jacobs 1999).

In writing in 1962 of the "invisible hand" guiding scientists toward discovery, Polanyi described mechanisms by which scientists take note of published results of other scientists, reacting to them, while in the case of the market, mutual adjustment is mediated by a system of prices. Scientists, like entrepreneurs, use a "limited stock of intellectual and material resources" as they exchange products and invent new ones, he wrote (Polanyi 1962 2000: 1, 3, 4).³ The result is not an egalitarian free-for-all of individual actions, or a democracy, but a naturally developing hierarchy of authority resulting from relationships of scientific apprenticeship, expertise, and judgment. The merits of scientific discovery are decided among experts, who are recognized for their mastery of the discipline, in what Polanyi called "chains and networks of overlapping neighborhoods" in which scientists accept and support the results of other expert peers (Polanyi 1962 2000: 7).

Polanyi downplayed in his *Minerva* essay the discrepancy between the free market emphasis on maximization of individual profit and the traditional scientific ideal of scientists working together in the common goal of universal truth.⁴ Herein lies a major tension between individualism and collectivism, whether at the group level or in the larger discipline. In addition, by invoking the image or model of the republic, Polanyi defended a governing hierarchy of elite authority within science in uneasy alliance with the free market of independent ideas and their consequences—a dissonant imposition of Plato's natural hierarchy of the republic on a "naturally" self-optimizing market (Thorpe 2009; Mirowski 1997; Fischer and Mandell 2010).

As a social epistemology, it is also striking that Polanyi's writings insisted on the centrality to scientific "discovery" of the "mental situation" of the independent scientist who, following his apprenticeship in a research group, establishes a line of research "which is his own personal, his own vital contact with nature" in the "ambition to secure credit by anticipating his rivals" while nonetheless recognizing that "all new knowledge becomes common property" in the "cooperation of independent minds" (Polanyi 1940 1997: 130–131). Polanyi's emphasis is very much on individual creativity and individual

autonomy within social structures. For Polanyi, “a small team of collaborators is merely an extension of the physical possibilities of the director of research; they help him in his experiments and carry out measurements set up under his supervision.” Reflecting on his own experiences directing a laboratory consisting of several sub-groups, Polanyi further cautioned “A leader of research who extends the number of collaborators to a point where he cannot actually see their experiments being performed, but has essentially to rely on their reports, is in danger of losing the solid ground of his work” (Polanyi 1940 1997: 132).

Ludwik Fleck developed a rather different model for the natural order of science. Fleck published the book *Entstehung und Entwicklung einer wissenschaftlichen Tatsache* (*Genesis and Development of a Scientific Fact*) in 1935 (Fleck 1979, 1980). Similarly to Polanyi, Fleck wrote of science as a system based in apprenticeship under an authoritative “thought style” practiced by a small esoteric circle of experts. That expert circle, in turn, intersects with other circles of scientists that Fleck, unlike Polanyi, extended to even more exoteric circles of non-specialists, all of which form what Fleck termed the scientific “thought collective.” Like Polanyi, Fleck drew upon his own career experiences and on some history, in particular, the 1906 discovery of a test for the diagnosis of syphilis. According to Fleck, August von Wassermann in 1921 wrongly claimed the so-called Wassermann test as his own creation. Instead, Fleck wrote, the discovery built on research by Octave Gengou with Jules Bordet at the Pasteur Institute in Brussels, as well as on Wassermann’s cooperation with Albert Neisser at the University of Breslau, and on collaboration in Berlin with the young Carl Bruck (Wassermann et al. 1906; Löwy 1993). “Epistemologically,” concluded Fleck, “the problem of discovery is insoluble from an *individualistic* point of view. If any discovery is to be made accessible to investigation, the *social point of view* must be adopted; that is, the discovery must be regarded as a *social event*” (Fleck 1935b 1979: 76).

The editors of the English-language translation of Fleck’s volume were the historian of science Thaddeus J. Trenn and the sociologist Robert K. Merton. Trenn explained their use of the English phrase “Thought Collective” for Fleck’s “Denkkollektiv,” by saying that Fleck had explicitly meant “collective” rather than “community” (Fleck 1979: xv–xvi). “Even now,” wrote Trenn and Merton in 1979, “there is perhaps no aspect of Fleck’s theory more potentially controversial than the perennial issue of ‘collective’ versus ‘community’ [i.e., *Kollektiv* versus *Gemeinschaft*], fraught as it is with implications concerning the status of individual personalities.” In a collective, wrote Trenn and Merton, “all the individuals are exchangeable, but the personalities of a community are not” (Fleck 1979: 164).

Indeed, a contemporary reviewer of Fleck’s book in 1936 explicitly objected to Fleck’s language of a thought-collective rather than a thought-community, or *Gemeinschaft*, claiming that Fleck was expressing a concept of

social life that was more Russian (read “socialist” or “communist”) than German (Fehr 2012: 83; Borck 2004: 455–456). Fleck had written in a 1929 article in *Die Naturwissenschaften* that “natural science is the art of shaping a democratic reality and being directed by it—thus being reshaped by it, [...] dependent on the collaboration and communication of many individuals, as many as possible” in thought collectives that undergo modification over time. He reiterated in a Polish article of 1936 that “[t]he modern scientific thought-collective ought to be called democratic,” distinct in form from hierarchical organization, a rather different point of view from Polanyi (Fleck 1929 1990: 245, 246, 1936 1986: 105). “The three factors involved in cognition,” Fleck wrote, are “the individual, the collective, and objective reality (that which is to be known).” The work eventually accomplished and the moments of discovery are “due to a kind of experience of the collective,” Fleck said, for which an individual may become recognized in the end as the “standard-bearer” of the collective (Fleck 1935b 1979: 40–42).

Just as there is tension between individualism and collectivism in Polanyi’s image of scientific practice, so, as noted explicitly by the philosopher of science Stephen Toulmin, there is tension in Fleck’s account between the role of the individual, which, for Fleck, is “the imprint of the provisional and the personal,” and the role of a community in achieving a collective fund of knowledge and an agenda for the future. Fleck sought to redress the imbalance between the personal and the collective, suggests Toulmin, by countering overly individualistic stereotypes of the nature of scientific endeavor (Toulmin and Stephen 1986: 271, 275). Discovery is a collective achievement in which results become scientific facts that are impersonal and self-contained.⁵

The Republic: Pauling at Caltech

In an article memorably titled “Nice Guys Don’t Win Nobel Prizes,” the American science journalist Natalie Angier wrote the following: “To thrive in science, you must be both a consummate collaborator and a relentless competitor. You must balance, with an almost gymnastic precision, the need to cooperate against the call to battle” (Angier and Natalie 1988). One of the great balancers on the high-wire tightrope between cooperation and competition was Linus Pauling (1901–1994), Nobel Laureate in Chemistry in 1954. Often called the greatest chemist of the twentieth century, Pauling was well known, too, for a charismatic rugged individualism that is stereotypical of the American western hero and for a strong competitive spirit (Nye 2000; Ceccarelli and Leah 2013).

Yet, Pauling worked in collaboration as often as he worked alone, and even his solitary-authored work depended heavily on collaborations. He spent his

most productive years at Caltech in Pasadena from 1922 to 1963, when he resigned after colleagues' criticism of his peace activities. He grew up in rural eastern Oregon, graduated in chemical engineering at Oregon Agricultural College in Corvallis, and completed his PhD in chemistry with Arthur Amos Noyes at Caltech, joining the faculty in 1927. He was a full professor by 1931 and Director of the Division of Chemistry and Chemical Engineering for over 20 years, from 1936 to 1958 (Hager 1995; Goertzel and Goertzel 1995; Nye 2007).

Pauling organized his research projects at Caltech into result-oriented research groups composed of graduate students, Research Fellows, and paid assistants (James and Jeremiah 2007: 68, 274). He had a research fund from Caltech, and, like many scientists, especially chemists, who were organizing team research in this era of the 1920s and 1930s, he sought and obtained external funds, such as grants from the charitable Rockefeller Foundation and Carnegie Institute. By 1938, 2 years after Pauling had become Division Head, there were 82 "research men" in the laboratories, including 17 faculty and staff members, 17 postdoc research fellows, and 48 graduate students, of whom 34 were working for the doctorate.⁶ As head of Chemistry and Chemical Engineering, he appeared to some of his chemistry colleagues to favor his own research interests over other areas of chemistry, which caused some rivalry and disquiet with colleagues (Roberts 1985: 62–64). Pauling supervised twice as many graduate students and postdocs as other faculty colleagues in facilities that included two large, multi-story, adjoining buildings (James 2007: 71–78; Hager 1995: 170, 203).

In organizing his research projects, Pauling, like many other laboratory directors, gradually introduced into his laboratories a broad range of increasingly specialized instruments that required expertise that he as laboratory director might not personally possess even if he wanted to make use of the new technique (Hackett 2005; Reinhardt 2006). Pauling's earliest research lay in X-ray crystallography and in the application of quantum mechanics to chemical bonding and chemical structure. His expanding personal agenda, however, required research groups in different areas of expertise, including X-ray crystallography, electron diffraction, electrophoresis, mathematical computation, immunology, the building of molecular models, and eventually computer programming. His assistants or collaborators, some of whom moved up the academic ladder at Caltech, were mostly undergraduate or graduate students, postdocs, and staff or faculty members.

Holmes Sturdivant, Pauling's first PhD student, became a salaried Research Fellow in 1930 and took over Pauling's previously personal research work of making X-ray diffraction photos, indexing diffraction patterns, and analyzing simple structures, eventually running the X-ray laboratory and instrument shop (James and Jeremiah 2007: 66, 279; 280; Pauling 1965: 7). Lawrence O. Brockway became Pauling's expert in building and running electron diffraction apparatus, first while a PhD student and then as Senior

Research Fellow. Brockway supervised graduate students and visiting researchers. After Brockway left Caltech in 1937, Pauling's PhD student Verner Schomaker directed the electron diffraction group for the next 20 years, becoming Professor of Chemistry in 1945 (James and Jeremiah 2007: 281, 283). Kenneth Trueblood, a 1947 PhD graduate, later reminisced that perhaps half of the papers published during the 1940s and 1950s from the "Gates and Crellin Laboratories" ended with a phrase to the effect "We are grateful to Professor Verner Schomaker for helpful discussion" or "valuable insights." Many graduate students registered formally for research credit with Schomaker or with other Research Fellows and Associates (Trueblood 1997: 526).

Another indispensable longtime collaborator was Edward W. Hughes, who joined Pauling in 1938 as a Research Fellow after meeting him at Cornell University in 1937. Hughes quickly became involved in structural studies of peptides, proteins, and amines, collaborating with Pauling and Robert Corey, among others, and introducing the techniques of the method of least squares into crystal structure determinations in 1940 (Hughes 1979).⁷ William N. Lipscomb, later professor of chemistry at Harvard University and 1976 Nobel Laureate, took his PhD at Caltech with Pauling in 1946. Lipscomb worked with Schomaker to master methods of electron diffraction studies of gas molecules, while Hughes taught Lipscomb X-ray diffraction methods, as was usual with graduate students in structural chemistry (Lipscomb 2002: vii). About this time, too, Pauling brought into Caltech the University of Chicago PhD and immunologist Dan H. Campbell, first as a Research Fellow and then as a faculty member, to study how antibodies work, while Pauling also was working on ideas about disease in relation to molecular shape in studies of hemoglobin and sickle-cell anemia with Harvey Itano. A recent M.D., Itano came to Caltech for a PhD, which he completed in 1950 (Gormley 2003).

For mathematical assistance, Pauling turned to graduate students such as George Wheland, Jacob (Jack) Sherman and E. Bright Wilson, Jr., the latter of whom co-authored with Pauling the 1935 textbook *Introduction to Quantum Mechanics with Applications to Chemistry* and began a distinguished career at Harvard University (Pauling et al. 1935; Pauling 1965: 11; Gordon et al. 1992). Sidney Weinbaum was Pauling's longest-term mathematical collaborator. After completing his PhD with Pauling, Weinbaum worked with Pauling as a Research Fellow from 1933 to 1943, carrying out calculations in X-ray and electron-diffraction analysis, as well as in quantum mechanics, on desktop calculators. He later assisted with mathematics for the work that became Pauling's papers of 1950 and 1951 with Robert Corey and Herman Branson on the spiral or helix structure of polypeptide chains. (Terrall 1991: 33; Goertzel and Goertzel 1995: 95–98).

Pauling also sought out collaborators who did not come to him as students or postdocs. One was the architect and artist Roger Hayward who in 1933 began making atomic and molecular structure illustrations for Pauling's

classroom lectures and textbooks. Hayward co-authored the beautifully illustrated *Architecture of Molecules* with Pauling (Heumann 2013). When Pauling became interested in protein structure in the mid-1930s, following reports of path-breaking protein researches by J. D. Bernal, Dorothy Crowfoot, and William Astbury in England, Pauling sought out expertise at the Rockefeller Institute for Medical Research in New York, where he persuaded the Institute to fund its own researcher Alfred Mirsky to spend 2 years at Caltech to help Pauling get his protein research going (Hager 1995: 196–197).

Certainly the most important of the protein collaborators was Robert B. Corey, another Cornell PhD who had collaborated with Ralph W. Wyckoff at the Rockefeller Institute from 1928 to 1937. Pauling arranged a research appointment for Corey, who brought his own equipment with him and became professor of structural chemistry in 1949 (Marsh 1997: 52–54). Using the X-ray method, Corey and his collaborators obtained fundamental information about the dimensions and structures of amino acids, peptides, and polypeptide chains, including the folding of chains and the alpha-helix and pleated-sheet structures of fibrous proteins. Corey co-authored thirty-three of his papers with Pauling, one of them Pauling's most cited paper of 1951 with Corey and Herman Branson. Another of Pauling and Corey's many co-authored publications was the ill-fated paper of early 1953 that too quickly proposed a triple helix structure for DNA.

While highly collaborative in his work, Pauling was fiercely competitive and protective of his name, discoveries, and theories. A student and colleague described him as an outsized personality who relished “both adulation and confrontation,” and Pauling was most at ease working directly with colleagues at his own institution, many of whom fell directly or indirectly under his supervision (Marsh and Richard 1997: 55). By the early 1950s his usual habit was to come early to the laboratory (or sometimes to appear at night), take care of administrative matters, talk with researchers, and spend the rest of the day working alone at home (Roberts 1990: 141).

Although Pauling said that his success lay in having lots of ideas and discarding bad ones, he doggedly fought against what he deemed others' bad ideas. He opposed for decades the molecular orbital theory developed by Robert Mulliken and Erich Hückel for describing the constitution of a molecule, even as chemists came increasingly to favor it over Pauling's atomic-valence theory of the chemical bond. Convinced of the accuracy of his helix-chain theory of protein structure, Pauling and his Caltech colleague Carl Niemann aggressively attacked the rival cyclol, or closed cage structure, proposed by Dorothy Wrinch. Pauling openly parted company with his former student David Harker when Harker not only questioned Pauling's resonance theory for benzene-type compounds, but also supported Wrinch against Pauling (Hager 1995: 223–234). In the 1950s Pauling objected to his former postdoc collaborator George Wheland's new interpretation of the theory of

the hybrid resonance structures, which Pauling and Wheland had first developed together in the 1930s (Park 1998: 27–28). The Harvard physical chemist George Kistiakowsky expressed a not uncommon outsider's view of Pauling's leadership, referring to Pauling's "advocacy of the doctrine of the infallibility of Pasadenean research" and his "pontifical style" (Kistiakowsky 1940: 457).

British objections that Pauling's alpha-helix models of keratin and collagen proteins did not fit Astbury's X-ray data contributed to a strong rivalry and competition in protein and nucleic acid research among Astbury's laboratory at Leeds and the research groups directed by Maurice Wilkins at King's College London, Lawrence Bragg at Cambridge, and Pauling at Caltech. The story is well known of Pauling's mistaken triple helix structure for DNA published in February 1953, followed by the announcement from Cambridge by Francis Crick and James Watson in April 1953 of their double helix structure. Competition had trumped cooperation in 1951 when Wilkins and Rosalind Franklin declined Pauling's request to see their recent nucleic acid photographs to aid his own research (Hager 1995: 398–399).

Pauling made good use of his resources in colleagues and facilities in building a reputation as a scientific genius. While at Caltech, by my count, Pauling published a total of 370 scientific papers and books, with 106 different co-authors on 175 co-authored papers. Twenty-three individuals co-authored three or more publications with Pauling (Petersen and Med 2006). Only 47 per cent of his Caltech publications were co-authored, and he was first author on 57 per cent of the co-authored papers, an unusually high percentage for first authorship among eminent chemists at mid-century. When questioned toward the end of his life, in 1994, about the physicist Herman Branson's recent claims that Branson deserved greater credit for the double helix papers published under combinations of the names Pauling, Corey and Branson, Pauling wrote: "[I]n some cases the fact that I had originated the work seems to me to be important enough to justify having my name as first author" (Goertzel and Goertzel 1995: 98; Hager 1995: 661–662). Of Pauling's ten most-cited papers, six appeared under his name alone, and four were co-authored with Pauling as first author.

The Collective: Dorothy Crowfoot Hodgkin at Oxford

Dorothy Crowfoot Hodgkin (1910–1994) was the first woman after Marie Curie to be awarded an unshared Nobel Prize in Chemistry.⁸ Pauling became aware of Hodgkin's research after seeing her 1934 paper, co-authored with her mentor Bernal, with the first successful X-ray diffraction photograph of a protein—pepsin. She and Pauling corresponded and met briefly in Oxford in early 1947, and she came to know Pauling better during her month-long stay at

Caltech on a traveling Rockefeller fellowship in November 1947, which was followed by Pauling's sojourn in Oxford during January to June 1948 (Ferry 1998: 234). There were short-term interchanges and visits, too, of junior researchers between the two laboratories, among them Barbara Low, Jack Dunitz, Jenny Pickworth Glusker, and John Rollett. In Pauling's controversies with Wrinch and with Astbury, Hodgkin was neutral, reporting that her own X-ray photos of the globular protein insulin did not support a decision between the cyclol and chain structures of protein nor between Astbury's and Pauling's keratin and collagen models (Hager 1995: 229; Ferry 1998: 147–160). Unlike Pauling, Hodgkin was not a scientist who arrived at quick conclusions or leapt into publication in order to forestall competition. If anything were to be published, it had to be correct, whatever the possibility of being preempted by another research group (Glusker 2011; Ferry 1998: 257).

Hodgkin's working environment was very different from Pauling's. She received a first-class degree at the all women's Somerville College in Oxford in chemistry and physics in 1932, writing an undergraduate thesis that resulted in her first published papers with the Oxford mineralogist and X-ray crystallographer Herbert Marcus Powell. Their study of the structures of thallium dialkyl halides followed up work done independently by both Pauling and John Desmond Bernal (Ferry 1998: 64; Powell Crowfoot 1932; Powell and Crowfoot 1934). Hodgkin received a Cambridge doctoral degree in 1937 with Bernal, who was a pioneer in demonstrating that X-ray crystallography data could decide between competing chemical structures. Hodgkin and Bernal optimistically wrote in a paper of 1937 that X-ray investigation, working in hand with chemical study, "can reveal the molecular structure with all the certainty of, and more refinement than, a chemical synthesis" (Crowfoot and Bernal 1937: 19). She married Thomas Hodgkin in 1937 but continued to publish mostly under her maiden name Crowfoot until the late 1940s (Dodson 2002: 185–186; Dodson et al. 1994; Hunter 2007).

A permanent Fellowship at Somerville College did not provide support for Hodgkin's research, and she shared laboratory space with her former advisor "Tiny" Powell in the makeshift mineralogy and crystallography space of the Old Chemistry Department in the Oxford University Museum of Natural History. Their research desks were in the same room in which Thomas Henry Huxley defended Darwin's evolutionary theory against the Bishop of Oxford in 1860. The eminent Oxford organic chemist Robert Robinson, who had taught Hodgkin as an undergraduate student, assisted her with an application to Imperial Chemical Industries (ICI) for monies to purchase badly needed new X-ray equipment (Ferry 1998: 90; Dodson 2002: 188). He also provided her with a sample of insulin crystals, which he had acquired from a drug company. Her initial work on insulin reported its molecular weight and its shape in her first single-authored paper in 1937 (Ferry 1998: 109–115). In the next years, her crystallographic structural studies focused on steroids, penicillin and other

antibiotics, Vitamin B₁₂, proteins in general, and in the 1960s, back to insulin. She became a Demonstrator at Oxford in 1945, a Reader in 1955, and the Wolfson Research Professor of the Royal Society at Oxford in 1960, four years before her Nobel Prize (Hunter 2002). In that year, 1960, her research group moved to newly built Chemical Crystallography laboratories in the Inorganic Chemistry Building. She retired from Oxford in 1977.

Many of Hodgkin's research collaborators began as undergraduate research students whom she tutored, for example, Dennis Riley, a student from Christ Church College, who later reminisced about the surprise at Christ Church when he chose to work with a young female Fellow in a women's college (Ferry 1998: 132–136). She set him to work on the structure of the two-amino-acid molecule diketopiperazine, which Corey became the first to solve in 1938. (Ferry 1998: 147). Hodgkin became adept at encouraging students and postdocs to develop their own skills in ways that would bring a particular expertise to her small group, whether it be mounting crystals and photographing them, using mathematical methods to elucidate diffraction patterns, applying chemical knowledge for structural analysis, thinking about structure from biological perspectives, or, eventually, writing and running computer programs rather than using trigonometrical methods and calculating machines.

Wartime brought changes, not only because Hodgkin began receiving Rockefeller Foundation funding for postdocs and equipment in 1940, but also because Bernal shipped his X-ray equipment to her when he became involved in the war, and several of his co-workers moved to Oxford to work with her. One was Harry Carlisle, who returned to Birkbeck with Bernal after the war (Hodgkin 1980: 46). With Hodgkin, Carlisle worked out the structure of the steroid cholesterol, pioneering the technique of using the presence of a heavy atom (in this case iodine in cholesteryl iodide salt) to better calculate a three-dimensional structure from a diffraction pattern (Ferry 1998: 189–190).

Like Pauling's group and other groups in X-ray and electron diffraction studies, Hodgkin was computing Fourier series of wave patterns as contour maps, and she was building models of wire and cork to visualize the positions of atoms. Calculations in the 1940 were aided by early electronic computers or punched card machines, for which her Somerville College student Barbara Rogers Low helped write a program. Low assisted in running the diffraction data at night on a machine in Circencester that was used in the daytime for tracking ship cargoes. Low also designed a device that could show three-dimensional molecular structures by looking at contours of electron density drawn on stacked transparent acrylic sheets (Ferry 1998: 115f., 189f., 204f., 211f.). Using these new methods, Hodgkin confirmed the correct structure of penicillin in 1945, with a research team that included Low at Oxford and Charles Bunn and Anne Turner-Jones at ICI. Their work decided in favor of the structure proposed by organic chemists Ernst Boris Chain and Edward P.

Abraham against a structure preferred by Robinson and Howard Walter Florey—all working at Oxford in cooperation and competition with each other (Williams 1990: 125–129). Robinson had not given up easily, however, initially suggesting that X-rays might have altered the structure of the molecule, which he had determined chemically (Ferry 1998: 214).

Hodgkin soon turned to the anti-pernicious anemia factor Vitamin B₁₂, which was crystallized first in 1948 and contained the heavy atom cobalt. By 1956 her group had a structure that was then the largest structural formula yet achieved (C₆₃H₈₈N₁₄O₁₄PCo). One of Hodgkin's principal co-workers was her undergraduate and then graduate student Jenny Pickwork, who collected and measured diffraction data, doing calculations slowly on an Oxford Hollerith-IBM punched card machine nearby. Pauling's group had possessed its own IBM computer since 1946. When Hodgkin and John White, a British scientist at Princeton, learned of one another's current work on Vitamin B₁₂, they agreed to combine their results and publish together. They concluded the work through cooperation with Kenneth Trueblood, who now was teaching at UCLA and visited Hodgkin's laboratory in 1953. He volunteered his students and a new UCLA Standards Western Automatic Computer (SWAC) for swift calculations. The Vitamin B₁₂ papers were co-authored with colleagues at Princeton University, UCLA, and Oxford (Ferry 1998: 249–262).

Hodgkin found herself in competition, however, with Cambridge University organic chemist Alexander Todd. She thought they were cooperating and had agreed to publish papers simultaneously in *Nature* announcing the structure of Vitamin B₁₂. When she learned that Todd was going to preempt publication with his announcement at a talk in Exeter in July 1955, she immediately boarded a train to the meeting and took the meeting floor after Todd's talk to explain her group's work. Todd told a *New York Times* journalist that his "team" had "won the race" with "assistance" from The Department of Chemistry at Oxford and a UCLA computer team. Although Todd subsequently sent a clarification to the newspaper, Jenny Pickworth later said, Todd seemed to view the X-ray crystallographic structural work as technical assistance rather than real chemistry (Ferry 1998: 262f.; Glusker 2011). Here, as in competition with Robinson, Hodgkin wanted proper credit for herself and for her group.

By the 1960s, Hodgkin's research group numbered some 12–15 doctoral students, post-docs, and senior research workers, plus clerical and technical staff, not one of them officially on the university payroll, but funded instead from outside sources including her Wolfson Chair research money (Ferry 1998: 297). Her work was cooperative both within Oxford and across institutions and the Atlantic. At this time, Hodgkin and her research team returned to insulin, feeding diffraction patterns directly into an Oxford-owned computer from punched tape. Her collaborators in this work included the postdoc Guy Dodson from New Zealand and the Australian Eleanor MacPherson

Coller, who soon married Dodson. When Hodgkin's group had an electron density map that she was ready to publish in 1969, Hodgkin insisted that her undergraduate collaborator Tom Blundell give the first presentation of the confirmed structure at an international meeting, even though, as Dodson noted, this presentation was "the culmination of 35 years of her research" (Hunter 2007: 388; Dodson 2002: 208; Ferry 1998: 324f.). In a letter to *New York Times* science journalist Walter Sullivan, Hodgkin commented on his having called insulin her discovery:

I am nearly 60 and by now have had a great deal of fame [...]. The crystal growing, and measurements on which the structure of insulin is based today were all made by members of my research group working with me in Oxford and particularly by the last four in, Guy and Eleanor Dodson, Tom Blundell and M. Vijayan [...]. The fact is [that] this kind of work needs many kinds of talents [...]. My part in the present insulin research has been largely one of discussion and of grant-raising for apparatus and to keep everyone going. Only for one glorious week, the last week of July, did I return to full-time research and help the others build the model (Ferry 1998: 326).

In the article in *Nature* that announced the structure of insulin, Hodgkin's name appeared eighth in a list of ten co-authors, which were not strictly alphabetical in order (Adams et al. 1969).⁹

As Crowfoot or Hodgkin, by my count, Dorothy Crowfoot Hodgkin published 66 single-authored papers, and she was first author on 34 of 114 multi-authored papers, co-authored with 118 individuals. Thirty-three collaborators co-authored three or more papers with her (Kademani et al.: 238-240). Thus approximately 63 % of her papers were co-authored, and her name appeared as first author on 30 per cent of co-authored papers, which is in general line with other eminent scientists of her generation, as studied in a sample of Nobelist laureates by Harriet Zuckerman (Zuckerman 1967: 392, 1968: 289). Thus Pauling is atypical of the sample of Nobelist laureates (since only 47 % of his papers were co-authored and 57 % of his co-authored papers were first-authored). Of Hodgkin's ten most-cited papers, all are co-authored, and her name appears first on only one of these papers.

Market Places of Competition and Collectives of Collaboration

The scientific work of Pauling and Hodgkin exemplifies the dependence of the modern professional laboratory on collaboration and teamwork within the laboratory, as well as cooperation and competition within and beyond a single laboratory or institution. It was this kind of collaborative science, locally based but dependent on the larger scientific community, that Polanyi and Fleck were describing in microcosm and macrocosm in their models of modern science. On the level of the research group, Pauling's style of scientific leadership and

research organization fits roughly into the metaphor of Polanyi's free-market republic of science, with its alliance between strong independent individualism and an elitist hierarchy of distinction and merit. In contrast, Hodgkin's style of leadership more closely approximates Fleck's image of collective science, with its practice of shared achievement in a democratic spirit, if not necessarily a democratic reality, where an individual leader becomes the "standard-bearer" for the collaborative group.

It has been suggested that Pauling's postdoctoral sojourn in Europe in the mid-1920s acquainted him with the command style of leadership found in the European "Herr Dr. Professor" who heads a hierarchically organized research institute. Pauling liked this model. After he became head of the Caltech Chemistry Division, Pauling consistently opposed decentering authority from the headship of a single person (James and Jeremiah 2007; Hager 1995). He also opposed structuring the Chemistry Division into autonomous departments of Inorganic, Organic, Physical, and Applied Chemistry, as was often done in similarly large laboratories, for example Max Perutz's Laboratory of Molecular Biology in Cambridge, where four division heads joined Perutz on a laboratory governing board (Roberts and John 1985; Ferry 2008: 195).

As we have seen, some of Pauling's faculty colleagues objected that he hired people into the Chemistry Division mainly to support his own personal research goals and not the good of the larger group (Hager 1995: 469; Roberts and John 1985: 54). Pauling gave considerable license to co-workers, but the independence did not extend to the practice of molecular orbital theory or to some physical methods such as low-temperature X-ray diffraction techniques, in which he was not interested. Having lost that battle while a graduate student at Caltech, Lipscomb pioneered the field of low temperature X-ray crystallography after leaving Caltech (Eaton 2002: 2–4).

During most of Pauling's career at Caltech, it was an all-male institution, including faculty and Research Fellows. He was more open-minded than some Caltech administrators and argued in favor of women as researchers in the 1940s and as graduate students in the 1950s. Dorothy Semenow, a student of Jack Roberts's, became the first woman PhD at Caltech in 1955. Pauling's wife Ava Helen and his daughter Linda each briefly assisted in the laboratory, as did his younger sons Crellin and Peter, with Peter completing a PhD in chemistry at Cambridge under John Kendrew. Although there were occasional parties at the Pauling home, with some graduate students becoming friends of the Pauling children, Pauling himself seems to have preferred to keep some formal distance between himself and his staff (Lipscomb: 133; Goertzel and Goertzel 1995: 83–84, 113).¹⁰ His graduate students and postdocs called him Dr. Pauling, while addressing some other faculty members by their first names (Hedberg 2013).

A significant number of students and postdocs had a close enough research relationship with Pauling that they remained at Caltech for most of their careers, among them Sturdivant, who became Professor of Chemistry and

Executive Officer of the Chemistry Division in 1958; Hughes, who retired in 1974 as Senior Research Associate; Weinbaum, who worked after the war at Caltech's Jet Propulsion Laboratory but ran into serious political and legal difficulty during the anti-communism purges of the early 1950s; and Dan Campbell, who flourished as Professor of Immunochemistry and became President of the American Association of Immunologists during 1972–1973 (Hammond 1972: 29; Terrall 1991; [Anonymous] 1974). By far the most eminent of Pauling's longtime Caltech collaborators was Corey, who was well known for his protein work and elected to the National Academy of Sciences in 1970. Pauling twice nominated Corey for the Nobel Prize, in 1961 and 1962, each time along with the names of Max Perutz and John Kendrew who shared the prize in 1962.¹¹

Among Pauling's longtime collaborators who permanently left Caltech, Lawrence Brockway joined the University of Michigan Chemistry Department in 1938, where he continued studies in electron diffraction, while Schomaker moved to Union Carbide Research Institute and then the Chemistry Department at the University of Washington (University of Michigan 2011; Trueblood 1997). Those who moved out of Pauling's orbit early in their careers often fared best in independent careers and personal recognition, among them Wheland, Wilson, Itano, Lipscomb, Trueblood, Dunitz, and others such as Alexander Rich, David Shoemaker, and Kenneth Hedberg.

In contrast to Pauling's laboratory, everyone in Hodgkin's laboratory was on first-name terms by her choice. Guy Dodson thought that this informal egalitarian tradition went back to Hodgkin's experiences in Bernal's politically leftwing laboratory, with whose sympathies for socialism and Communist regimes she agreed (Dodson 2002: 189; Perutz 1994). Most of Hodgkin's undergraduate students and many of her graduate students were women, although men from Oxford colleges and from outside Oxford did research under her direction. Hodgkin's babies and her coworkers' children sometimes were seen in the laboratory when home caretakers were not available (Dodson 2002: 189; Gluster 1994). Her management style most often was indirect, asking questions such as "Don't you think it might be interesting to try..." or "Wouldn't it be nice to do so and so?" (Ferry 1998: 318). While Pauling was described as charismatic, Hodgkin was said to have "magic about her person. She had no enemies, not even among those whose scientific theories she demolished or whose political views she opposed" (Perutz 1994).

If Hodgkin was more democratic and egalitarian in leadership style than Pauling, she was not keenly interested in all work proposed or undertaken by her protégés, but she did not stand in their way. Dodson later commented on Hodgkin's "nervousness about her mathematics," but she welcomed and recognized the mathematical skills of some of her students (Dodson 1994: 194). One of her most gifted mathematical PhD students was David Sayre, who completed his degree in 1951 while accomplishing novel work at Oxford in

solving the so-called “phase problem” in crystallography that results from the loss of phase information in the measurement of diffraction intensity. Sayre helped develop what became known as direct methods, based in statistical techniques, in crystallography for small molecules, and he went on to become part of the Watson IBM Research Center in New York. According to Georgina Ferry, Sayre said of Hodgkin that “Dorothy” “did not have a symbolic, mathematical mind, she had a much more concrete mind.” She thought in terms of maps and images with an intuition based in experience that amazed her coworkers. Sayre returned for a sabbatical in the early 1970s to Oxford, which he still found hospitable and congenial (Ferry 1998: 244; Kirz and Miao 2012).

Former students such as Guy Dodson, as well as senior colleagues like Perutz who often visited her laboratory from Cambridge, all commented on what Dodson called “her deeply held sense of equality” and her co-workers’ feeling that “we were an extension of her family” whether in the lab, a nearby pub, or her home (Dodson 1994: xxvi–xxvii). Hodgkin said that the work she wanted to accomplish is not easy and “needs the combined experience and cooperation of many, if success is to be achieved” (Hodgkin 1964: 330). In her Nobel Lecture of 1964 she told her audience that her research owed a great debt to the work of “my colleagues who have provided many of the ideas I have used and many interesting examples of similar analyses, my collaborators, without whose brains and hands and eyes very little would have been done.” Of the 29 references in her printed Nobel lecture, only nine are for papers that she wrote, all as co-author, in contrast to Pauling’s printed Nobel Lecture with 13 references, all for papers he wrote or co-authored. He did not explicitly mention cooperation or collaboration (Hodgkin 1972: 90; Pauling 1964: 433).

Some of Hodgkin’s co-workers, such as Guy and Eleanor Dodson stayed in Hodgkin’s laboratory for a considerable time as Research Fellows, while others left sooner, Riley, for example, for the Royal Institution. Barbara Low had a postdoctoral appointment at Caltech in 1945 and concluded her career as Professor of Biochemistry and Molecular Biophysiology at Columbia University. After a postdoc year at Caltech, working directly under Corey, Pickworth and her new husband Donald Glusker moved to Philadelphia, where she spent her career from 1956 at the Fox Chase Cancer Center, serving as its head from 1966 to 1979. Pickworth-Glusker received the Garvan-Olin Medal of the American Chemical Society in 1979 and the 1995 Fankuchen Award of the American Crystallographic Association in 1995 (Glusker 2011). Marjorie M. Harding, a 1961 PhD who worked on insulin, became head of X-ray crystallography at the University of Liverpool. The Dodsons moved from Oxford to the University of York in 1976 where they set up a new laboratory. He was elected FRS in 1994 and she in 2003 (Baker 2013). After his PhD, Blundell joined the University of

Sussex in 1973 and, following other faculty and administrative appointments, he became the fifth Sir William Dunn Professor of Biochemistry and head of department at Cambridge. Blundell was elected FRS in 1984, and he was knighted in 1997 (Blundell 2014).

The credit that Hodgkin's coworkers received early in their Oxford research years stood them well in their continued careers, and many of her women students became pioneers for women scientists during the 1950s and 1960s. One undergraduate who took a 1947 bachelor's degree under Hodgkin's supervision, but soon abandoned chemistry and ended her career as British Prime Minister, was Margaret Roberts Thatcher.

It is tempting to attribute differences in Pauling's and Hodgkin's styles, including their contrasting preferences for more individualist or more collective leadership, to gender, but that cannot be the whole story. Not all men behaved like Pauling; not all women behaved like Hodgkin. The intellectual and social dynamics of Hodgkin's research team, however, composed as it was of women and men working together on equal terms, contributed to a strongly collective spirit of achievement in what Georgina Ferry calls an "egalitarian" and "collaborative" culture (Ferry 2014). This culture partly had its roots in Hodgkin's experiences in Bernal's laboratory. It also had institutional roots in Oxford's more limited resources for the sciences than a place like Caltech and in the difficulties faced by a female scientist in a women's college among an overwhelmingly university-wide male faculty. Hodgkin had to be more cooperative with researchers outside her laboratory, who also were more geographically dispersed across different institutions and continents, than was necessary for Pauling.¹²

Fleck spoke of doing science in a democratic way and it should be remembered that in the early period of modern science, when the Paris Academy of Sciences was founded in 1666, its initial organization was along the lines of Francis Bacon's prescription to submerge individual egos to the general good. As a consequence, the Academy gave credit for individual members' contributions to the Academy as a whole. The Paris Academy abandoned this formula in 1699, however, and began assigning credit to individual scientific contributors, as had always been true of the Royal Society in London (Hull 1988: 312f.).

The notion of collective science and collective credit was seldom revived in the next centuries. One perhaps unique example of collective science in the twentieth century is the reconceptualization of the foundations of pure mathematics begun in the 1930s by the nine Paris-based mathematicians who started publishing the multi-volume *Elements of Mathematics* under the pseudonym Nicolas Bourbaki. The members were known as "collaborators" of the non-existent Bourbaki. They *were* Bourbaki. They conducted public seminars but kept their exact membership secret, circulated chapters privately among themselves, and were rumored to keep Bourbaki

eternally young by replacing any of their ten male members when he turned fifty (Aubin 1997; Beaulieu 1999, 2006). Many of the one-time collaborators individually received major recognition in mathematics, including the award of more than fifty Field Medals to members through time (Michon 2015). When, however, “Bourbaki” received the Cognacq-Jay Prize in 1967 from the French Academy of Sciences, four of the nine founding members—Henri Cartan, Jean Delsarte, Jean Dieudonné, and André Weil—accepted it (Beaulieu 1999: 221).

Just as the Bourbaki collective sometimes had to be represented by a few of its individual members, so, too, the Lunar Sample Preliminary Examination Team had to have a “standard bearer.” In 1970, their collaborative papers on the analysis of lunar rocks (retrieved in the July 1969 moon landing) began to appear. A paper published in the weekly journal *Science* was authored by “The Lunar Sample Preliminary Examination Team,” and the sixty-two members of the team were listed alphabetically by name in a footnote. When summarized in *Chemical Abstracts*, however, the team designation disappeared in favor of the name of the first co-author in the *Science* article, along with ‘et al.’ representing the other sixty-one team members (Piternick 1985: 24; The Lunar Sample Preliminary Examination Team 1971). Part of the rationale for such designation is that a standard bearer for the group is required to accept public responsibility as well as public recognition.

By contrast, some 40 years later, when two experimental teams at CERN’s large hadron collider simultaneously announced the experimental confirmation of the Higgs boson in July 2012, formal authorship was collective. The teams, each composed of thousands of members affiliated with hundreds of different institutions, simultaneously announced their “observation” of the particle in the same issue of *Physics Letters B*, one team under the authorship of the “ATLAS Collaboration” (led by Fabiola Gianotti) and the other under the name of the “CMS Collaboration” (led at that time by Guido Tonelli). The names of ATLAS team members, including Gianotti, were listed alphabetically, and CMS team members were listed alphabetically within institutional affiliations, with Tonelli listed alphabetically under the University of Pisa (Overbye 2013; ATLAS Collaboration 2012; CMS Collaboration 2012). Earlier teams searching for the Higgs boson used authorial names such as the ALEPH Collaboration, the DELPHI Collaboration, and the LEP Working Group for Higgs Boson Searches (ALEPH Collaboration, DELPHI Collaboration, L3 Collaboration, OPAL Collaboration, and the LEP Work Group for Higgs Boson Searches 2003).

In recent high energy physics, then, the collective has been achieved in a fashion clearly unimaginable to Polanyi, who doubted in the 1940s that a research group could function with many more than half a dozen or so members under the direction of its research leader. Polanyi did not think of science as a democratic enterprise, nor did Pauling. As we have seen, Hodgkin

adopted something like Fleck's notion of collective teamwork, described by Fleck in analogy to "a soccer team" (Fleck 1935b 1979: 46). On this analogy, collaborative research at Caltech often resulted in outsized credit for the team captain, while at Oxford the captain tried to highlight the skills and contributions of all members of the team. The Hodgkin culture led to relative ease for team members who wanted to leave the captain in order to establish their own independent careers, while the Pauling culture often kept important team members in the shadow of their leader. In both groups, while there may have been some rivalries of individuals within the group, or competition for funds or for new students to support different instrument techniques, most competition occurred extramurally with other research groups, although less so for Hodgkin than for Pauling. Neither the individualist hierarchical republic of science nor the more egalitarian thought-collective adequately describes the reality of scientific life, but the laboratories of Pauling and Hodgkin offer food for thought about the balance in scientific life between individualism and collectivism, competition and cooperation, and how credit is given for scientific discovery and achievement.

Acknowledgments

I am grateful to Maney Publishing and to the Editor of *Ambix* for permission to republish some parts of the article, Mary Jo Nye, "Mine, Thine, and Ours: Collaboration and Co-Authorship in the Material Culture of the Mid-Twentieth Century Chemical Laboratory," *Ambix: The Journal of the Society for the History of Alchemy and Chemistry*, 61, #3 (August 2014): 211–235. For helpful archival assistance, I thank Chris Petersen and Trevor A. Sandgathe at the Oregon State University Special Collections and Archives Research Center, and Charlotte E. (Shelley) Erwin and Loma Karklins at the Caltech Archives.

I presented earlier versions of this paper at the September 2014 annual meeting of the Deutsche Gesellschaft für Geschichte der Medizin, Naturwissenschaft und Technik in Munich and at an April 2015 History of Science Colloquium at UCLA. I am grateful to colleagues for comments and criticisms on those occasions, especially to Sharon Traweek who reminded me that I should discuss teamwork in recent physics. I also appreciate advice and help from Ursula Klein, Robert A. Nye, anonymous referees, and the editors for this *NTM* issue, especially Kärin Nickelsen.

Endnotes

- 1 Since around the time of Descartes, the notion of the individual self as the source of knowing had begun to thrive in the cult of the solitary author. As described by Michel

- Foucault, the coming into being of the notion of “author” was a “privileged moment of individualization” in the history of ideas and literature, including philosophy and science (Lunsford and Ede 1990: 79, 80–81, 88; Foucault 1979: 115). Yet few of the great heroes and solitary authors of early modern science in fact worked in isolation, although great scientists such as Isaac Newton often tried to claim laurels for themselves alone, despite “standing on the shoulders of giants,” as Newton put it in a letter to Robert Hooke (Merton 1965: 1).
- 2 There are few studies focusing explicitly on comparison of Polanyi and Fleck. For one such analysis, comparing Polanyi’s theory of tacit knowledge and Fleck’s theory of thought styles, see Hagner 2012.
 - 3 Friedrich Hayek spoke of spontaneous order and unintended consequences in his inaugural lecture at the London School of Economics in 1933 and extended the notion to general knowledge in his presidential address in 1936 before the London Economic Club, published in the LSE journal *Economica* (Caldwell 2004: 197, 238). The notion of the “marketplace of ideas” was invoked by US Supreme Court Justice Oliver Wendell Holmes in 1919 in his statement that “the best test of truth is the power of the thought to get itself accepted in the competition of the market” (Pigliucci 2010: 112).
 - 4 The free market metaphor easily leads to the notion later invoked by sociologists such as Pierre Bourdieu or Steven Woolgar and Bruno Latour of competitive scientific systems in which a winner takes the gains through cycles of intellectual and social credit and accumulation, no matter what the truth of the matter in the natural world (Bourdieu 1988; Latour and Woolgar 1979). See my discussion in Nye 2011: 297–301.
 - 5 Polanyi seems to have been unaware of Fleck’s published work until the 1960 s, although Fleck refers in a posthumously published article, which Fleck wrote in 1960, to their “often similar statements about science and society.” However, writes Fleck, without specification, Polanyi’s “ultimate inferences are quite different” (Fleck 1960 1986: 156).
 - 6 This information is found in Pauling’s two-page document dated 28 September 1938 and available at Linus Pauling Online (Oregon State University Special Collections and Archives Research Center), URL: <http://scarc.library.oregonstate.edu/coll/pauling/calendar/1938/09/28.html> (last accessed: 10.01.2016).
 - 7 In the method of least squares, the best fit to data that overdetermines the system is obtained by minimizing the sum of residuals, where a residual is the difference between an observed value and the fitted value provided by a model.
 - 8 Marie Curie received the chemistry award in 1911 (after sharing the 1903 Nobel Prize in Physics with Henri Becquerel and Pierre Curie). Her daughter Irène Joliot-Curie shared the chemistry prize with Frédéric Joliot in 1935. Hodgkin received the chemistry award in 1964.
 - 9 In counterpoint to Hodgkin’s modesty, Guy Dodson told her biographer Georgina Ferry that Hodgkin’s insight into protein structure, electron density and how these two aspects of crystallography came together were as good as anybody’s. And she built that model with me—Dorothy, [Mamannamana] Vijayan and I built it together, and I wouldn’t say her contribution was any less than mine for a moment. [...] I have no difficulty in saying she solved insulin. She set up the group—in a sense, we were the experiment” (Ferry 1998: 327).
 - 10 Pauling personally supported the appointments of some women as researchers before women were officially admitted to Caltech as graduate students in 1953 (Hughes and Edward 1979: 28). Women were first admitted as undergraduates in 1970.
 - 11 Corey’s other Nobel nomination came from Jürg Waser, a former Pauling PhD student who returned to Caltech in 1958 as Professor of Chemistry (Nobel Prize Nomination Database).
 - 12 For good overviews of some of the issues of gender and women in science, see Holton and Sonnert (1995) and Watts (2007). On Oxford, Morrell (2009) and Williams (2009).

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